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SCIENTIFIC RESEARCH IN RELATION TO AGRICULTURAL PROBLEMS[1]

I AM proud of having an opportunity to assist at the birth of this new society. In this case it seems that parturition has been long overdue—perhaps owing to the size of the embryo, if one may judge by the secretary's statement of membership. At any rate it proves to be a very vigorous healthy youngster and since it will be watched over by all these experts in nutrition, diseases, etc., one can safely predict for it a rapid growth and active maturity.

I must ask you not to estimate my modesty by the title of my remarks, which appears on the program—"Scientific research in relation to agricultural problems." This very large subject has been assigned to me by your committee and one consents to discuss it on the present occasion only because the relationship of scientific research to the industries *in general* has recently been thoroughly discussed and forcibly brought home to every one. The relationship of scientific research to agriculture is only a particular phase of this general question which has been frequently and ably discussed from many quarters. It would serve no useful purpose to point out particular agricultural examples of principles which should be familiar to all scientists. It will, therefore, not be necessary to attempt a comprehensive treatment of the subject. It will be sufficient to point out certain respects in which the relationship is peculiarly important to agriculture, or in which agriculture differs from the other industries. In accordance with the desire of your committee, I shall speak largely from the standpoint of the so-called pure scientist.

Agricultural scientific research suffers more

[1] Address delivered at the organizing convention of the Canadian Society of Technical Agriculturists, Ottawa, June 3, 1920.

than does any other form of industrial research from a lack of realization on the part of the general public of its possibilities. This is due, I believe, to the familiarity on the part of the majority of people with agricultural operations. Any one will concede the value of research in relation to manufacturing industries involving chemical actions. The great majority of people are innocent of any knowledge of chemistry, and regard it as a highly complex, mysterious study capable of performing all sorts of wonders—in which opinion they are not discouraged by the chemists. But every one thinks he knows something of agriculture and many people think they are experts at it. To most people agriculture is simply taking a little care of plants and animals which would grow anyway. They can see no need of investigation in such familiar operations as ploughing, harrowing, harvesting and thrashing. And it is through average public opinion that the expensive support for research must be obtained. The head of an agricultural college informs us that it has just taken him three years to convince his board that a plant pathologist has any useful duties to perform.

Of course all technical agriculturists know that agriculture, like the other industries, has reached a stage at which little progress can be expected from casual observation or ordinary experience. Progress will result only from the careful application of scientific facts and principles which are known only to those who have been properly trained, or which have not yet been discovered. Agricultural problems are just as difficult and complex as those of any other industry. Our confidence that great progress will be made by the application of scientific principles in agriculture results from our knowledge of what has been accomplished in this way in the past. The achievements of agricultural scientific research in actual financial benefits to the country are not surpassed by those of any other industry. This is not the occasion, however, either to mention the problems which have been solved or to point out those which can be solved by the application of scientific principles. Owing to the fact that such a large proportion of our population must always be engaged in agriculture, any advance through scientific work must result in benefits which in the aggregate surpass those in other industries.

My second point is that technical research in agriculture involves research in an unusually wide range of basic subjects and that the technical researcher in agriculture is therefor peculiarly dependent on so-called pure science. Agricultural problems involve, among other things, the physics, chemistry, biology and geology of the soil, physiology of plants and animals (itself including many basic subjects), pathology of plants and animals, systematic biology, including entomology, foreign plant introduction, genetics, bacteriology, mechanics, climatology, sociology and economics. Many problems involve several of these subjects. There are, of course, routine problems such as testing varieties, rotations, etc.; which may involve little more than general agricultural knowledge. But the problems which will result in new departures of importance are likely to demand a profound knowledge of basic subjects. The big advances requiring only superficial science have mostly been made, and progress in future will depend more and more on profound study in more than one basic subject. The workers in these basic subjects supply the raw material—scientific information—which the technical agriculturists work up into manufactured articles—better agricultural practises. To sustain our metaphor further these manufactured articles are transported by the agricultural educationists to the consuming farmers.

Of course, as in other cases, the workers in these different fields ("fields" is surely a good word in an agricultural discussion) can not be sharply segregated. Many technical men engaged primarily in improving practises are also spreading information among the farmers; many of them are making discoveries in theoretical science. The same piece of work may involve both discovery and application. To use a simile from our own subject, the technical agriculturist may be considered a cross between the so-called pure scientist and the farmer. And as in Mendelian crosses, there

are degrees of dominance with respect to the contrasted characters of the parents. Sometimes the technical man is mostly pure farmer, sometimes mostly pure scientist, sometimes exactly intermediate. Not rarely the cross proves too wide and the offspring are completely sterile.

Owing to the unusually wide range of basic subjects this relationship of pure and applied science is peculiarly important in the case of agriculture. It is hopeless to expect a technical agriculturist to keep thoroughly posted in all the subjects ancillary to agriculture; it is equally hopeless to expect a worker in an ancillary science to keep posted on practical agriculture. There is need of special machinery to stimulate discoveries in those parts of the basic subjects related to agriculture and to provide for their immediate application.

The solution of this difficulty and others presently to be mentioned depends, in my opinion, on a large degree of organized cooperation in research. All the reasons in favor of cooperation in industrial research in general apply in the case of agriculture and these strong additional reasons.

The peculiar dependence of technical research in agriculture on research in a wide range of basic sciences demands cooperation between the pure scientist and the technical man. A technical agriculturist endeavoring to solve a practical problem is very likely to encounter a fundamental problem. A fundamental researcher is very likely to uncover a fact or principle which can be immediately applied—if he knows enough of the practical to make the application. Sometimes a technical man's duties and interests and training will permit him to follow up a fundamental lead. Sometimes a worker in a fundamental subject, may know enough of the practical and possess the inclination to make the application. But such cases are likely to be relatively rare. It seems obvious that the best results both from the practical and theoretical standpoints are to be obtained by close cooperation of all concerned. The practical man knows the problems and discovers where fundamental information is lacking in order to solve them. He can then enlist the services of the pure scientist in those problems which his duties, or training or interests will not permit him to attack. At the same time—and this is not the least important effect—theoretical science will be advanced. There will be a complex series of effects and counter-effects of the theoretical on the practical. It can not be too strongly emphasized that theoretical science receives as much from practical science as it gives to it. The theoretical advances made in practical research are as important as the practical advances made by the application of theoretical principles. And in agricultural problems the possibilities are particularly great.

I should like to refer to a particular example of what I have in mind. A very practical problem is the breeding of disease resistant plants. In the case of stem rust of wheat it turns out that the problem involves fundamental genetical work. But the prosecution of it requires a very substantial knowledge of plant pathology and its methods; it can be carried on best in collaboration with plant pathologists. Moreover it has recently given a very valuable lead in fundamental botany by showing the existence of many physiological and geographical races of rust which can be distinguished only by their virulence in particular varieties of wheat. Stakman and his collaborators have proven the existence of many of these geographical races in the Northern States and work at Saskatoon this year has demonstrated the existence of several scattered over western Canada, as well as the occasional occurrence of more than one race in the same locality. This discovery seems for the time to have made a practical solution impossible. But the point is that the geneticist and plant pathologist have had to collaborate on the problem and both have uncovered important leads for theoretical research. And before economic results are obtained the practical agronomist must be enlisted to test yields, earliness, etc. This investigation also shows the necessity of having the same work carried on concurrently at different places to meet varying local conditions.

Many of the most important problems in agriculture will in this way require the united efforts of several kinds of researchers. This must be true from the very nature of agricultural problems involving as they do a highly complex sort of conditions and a very wide range of basic subjects. Without cooperation we find the technical man necessarily spreading his efforts over so many subjects that he accomplishes little. This result is already too common. Without cooperation we find the pure scientist making foolish practical claims or helpless before an unusual practical situation. Nor will the best results be obtained when the cooperation consists of the fundamental scientists acting as chore boys for the technical men—performing their chemical analysis, physiological experiments or bacteriological identifications.

Apart from the need of workers of several kinds, many problems are so big that even where only one kind of researcher is necessary, several of these must collaborate if results are to be obtained in reasonable time. Again, in Canada local and environmental conditions are so highly varied that in the case of problems of nation-wide importance work must be carried on concurrently at many places.

With the argument that cooperation is necessary in order to avoid duplication of effort I have no great sympathy. We need both collaboration and duplication or collaboration *by* duplication. We all know of too many cases where claims have been made which have not been justified by the scientific work, which have injured scientific agriculture, and which would never have been made by several men working together. The cry of needless duplication of effort has been overdone; until agriculturists become infallible we can stand a great deal more duplication. It should, however, be deliberate duplication knowingly undertaken, not that of several men working in ignorance of each other's efforts.

That organized cooperation in research can accomplish a great deal when support is available, has been abundantly proven by the war experience of all nations.

In this society we have the machinery at hand to bring about the needed cooperation. This society will soon bring together all Canadian technical agriculturists and, I hope, all researchers in fundamental subjects whose work is in any way related to agriculture. Projects to be attacked can here be divided into phases and each man assigned the phase in which he is best qualified or most interested. Varying local conditions can be met by providing for the concurrent prosecution of similar work in different places. The groups must be democratically organized and cooperate freely; they can not function if one man assigns tasks to others. There is no need of entering into details at this time. They can be worked out to suit the problem.

Such a program will require frequent group meetings for consultation, formulating and modifying plans, reporting results, etc. These meetings can best be held in connection with the conventions of this society. On account of the great distances involved, it will be necessary that the institutions to which these men are attached should pay their travelling expenses. I believe that little difficulty will be encountered in this respect. Certainly these institutions could spend their money in no way which would be likely to yield more valuable results, not only in actual problems solved but also in keeping their staffs efficient.

That a program of consultation and collaboration will receive wide support is shown by the resolutions passed at the conference of deputy ministers of agriculture recently held at Ottawa. A resolution concerning experimental farms passed at that conference reads, in part, as follows:

> Be it resolved that . . . definite measures be adopted that will bring about greater cooperation in planning, conducting and giving publicity to the results of experimental farm work, to wit:
>
> (*a*) The formation of joint provincial advisory committees to be composed of representatives from the Dominian and provincial departments of agriculture. . . .
>
> (*b*) That such provincial committees shall meet not less than once annually to consider the results of experimental work . . . for the preceding year or years and to discuss and as far as may be pos-

sible recommend and approve contemplated lines of experimentation for ensuing years with a view to building up a system under which the work of the respective stations shall, as far as possible be supplementary to and correlated with each other. . . .

(*c*) That from time to time . . . conferences of these committees or representatives thereof from groups of provinces or all the provinces be held. . . .

Another resolution with respect to plant breeding reads, in part, as follows:

Be it resolved that plant breeding work in Canada whether carried on under federal or provincial direction should be fully coordinated and correlated by the holding of conferences of Canadian plant breeders . . . that the mode of extension (of plant breeding work) be arranged by conference from time to time between federal officers and the officers of the provinces concerned so as to avoid duplication of effort and attain the maximum degree of efficiency.

It is of course possible that local jealousies and personal affairs may interfere with the working out of such plans. I have heard of an institution where one kind of worker may study an insect so long as it remains in the air but as soon as it penetrates the hide of a cow he must not seek to follow it; it then becomes the property of a different subdepartment. If it digs into the ground another subdepartment claims it. I know that some technical agriculturists are constantly on the watch lest a botanist or chemist tread on their preserves, and that some pure scientists have "no trespassing" signs up for technical agriculturists. When members of the same department quarrel over such things we may expect trouble in a larger organization. The division of credit for work done may cause some heartburnings. But I believe the good sense of all concerned and the desire to get results will overcome all such difficulties. And a little experience in cooperative work will soon convince every one of the essential decency of scientific colleagues.

Such work need not interfere with any one's initiative, and no matter how much cooperation may be secured in solving practical problems, undoubtedly much of our progress will continue to depend on individual brains and initiative and imagination. Cooperation can not replace intellect but it can make intellect much more effective in solving certain kinds of problems.

Moreover it must be remembered that cooperation can be effective only with certain kinds of problems. The problems must be clean-cut, easily outlined. The end sought must already be perfectly clear; the probability of success must be demonstrable; the methods of attack must be fairly obvious from the data at hand. But there is another kind of studies—those which really open up new fields of interest and importance. These studies must be highly individual and do not lend themselves to cooperation. Such problems can not be outlined because they are not known or are perceived only vaguely. One gets a hint but the end of the trail is not clear; the success of the work is doubtful or may appear ridiculous. The project can not be outlined so clearly and attractively as to enlist the help of colleagues or the support of executive boards which control funds.

And just here, it may be remarked, is a defect inherent in all institutions and organizations whose specific purpose is the carrying on or support of research. The directors or chiefs can not give financial support unless the problem can be clearly outlined, and the probability of its solution demonstrated. They must thereby exclude many of that second and higher type of researches to which I have referred. If Mendel had had to ask the prelate of his monastery for land and labor to carry on his experiments, he could not have justified his request by predicting the discovery of his law of heredity. There is after all some advantage in holding a teaching position in which one can potter at whatever notions one conceives without having to justify them to some one controlling his salary and without having to give reports at intervals. We teachers are not as envious of you full-time research men in governmental positions as you might suppose.

For the same reason it would, in my opinion, be a mistake to do anything that would tend to concentrate research in governmental departments or institutions. Every teacher

should be encouraged to work at whatever problems he wishes to attack. When a man's teaching or other duties are sufficient to justify maintaining him and yet leave him time for research he is more likely to choose problems which break new ground than if every research has to be justified even for the reason that he is paid a salary for research.

There must in general be no restriction on a man's choice of problems or on the distance to which he may follow a research lead. For this reason I must object to the suggestion made to-day that provincial men confine themselves to problems of provincial importance and that problems of wider significance be reserved for the Dominion Department of Agriculture. One never knows where a trail is going to lead—that is one of the chief attractions in investigation work. If a worker in Saskatchewan uncovers a trail which leads over in Manitoba or Ontario or the States, he won't stop at the boundary. The wider its significance the harder he will follow it. Our most important provincial problems are of equal importance outside provincial boundaries. You can not say to a provincial man "You may study these little local questions, but you must leave the big things to the Dominion men at Ottawa." Only a very mediocre set of men would endure such a restriction.

Apart from helping by cooperation in the solution of practical agricultural problems, the worker in a basic subject can do a great deal by a judicious choice of problems and materials. A geneticist or plant physiologist for example in attacking a fundamental problem can use a crop plant of great economic value just as well as the usual greenhouse plants. At the same time he is likely to reveal, perhaps incidentally, perhaps directly, information of great practical importance concerning this plant. Our scientific men could attack these problems on the borderline between the theoretical and the practical. They could attack a practical problem not only for its own sake but in the full expectation of uncovering a theoretical lead. In many cases in the past they have not attacked practical or semi-practical problems partly because of ignorance concerning them, partly for fear of offending technical colleagues.

Another difference between research in agriculture and that in the other industries concerns the remuneration of the workers. The industrial research man shares, in part at least, in the financial benefits which accrue from his work. Great increase in wealth results from the perfection of a technical invention. Even if the researcher is a hired employee of a corporation he shares handsomely in the benefit. The industrial researcher therefore always feels the powerful financial stimulus.

The agricultural researcher, on the other hand, deliberately renounces all such rewards. In that respect he is like the pure scientist. Though his work may result in great financial benefit to his country, he knows that he will profit not at all or very slightly. From the nature of his work he must be attached to governmental or educational institutions, and he knows that the salaries in such positions can never be very large. But when for the sake of his work he renounces hope of becoming wealthy, he can surely expect a reasonable salary. If good researchers are to be retained in and attracted to agricultural work, the remuneration must be sufficient, not to compete with what other industries offer to research men, but to make possible comfortable habits of life. Our standard joke concerning the salaries of teachers is unfortunately just as applicable to positions in which the teachers are also researchers and to those in which research only is carried on.

There is another aspect of the relationship between scientific research and Canadian agricultural problems to which I must refer, namely, the educational aspect. It should be perfectly clear that the men who are to do worth-while investigational work in agricultural problems must have a thorough training in the basic subjects as well as a broad education in the languages and humanities. As I have already pointed out, a thorough understanding of agricultural questions demands a peculiarly broad acquaintance with many fundamental subjects. The problems of agriculture involve just as complex scientific con-

ditions as do those of medicine or engineering for example. And the men who are to solve them must have just as broad and thorough a fundamental training as the researcher in medicine or engineering. Moreover, apart from his research, the technical agriculturist has to face just as many situations requiring broad education and culture as have any other professional men.

Now the medical colleges require the equivalent of two years of arts work with specified large credits in the sciences, before admission is granted to the purely medical studies. Some indeed require full arts graduation and all medical authorities advise graduation in arts even where they do not require it. All this is required of those who are to become only general medical practitioners. Much more is necessary for the research man.

Similar conditions are found in regard to the training for other professions. In engineering, law, divinity, a broad fundamental training is considered necessary and is generally required. At the recent Canadian universities conference held at Quebec a resolution was unanimously adopted calling for large increases in English, history, economics and particularly fundamental science in the training of engineers. The ideal of all educationists in these professions is to secure complete arts graduation before admission to professional studies, and failing that a large and specified amount of arts work.

In my opinion the professional agriculturist should have just as thorough a pre-professional training. This is true not only for the researcher in agricultural problems but also for agricultural teachers in schools or colleges, district representatives, inspectors, laboratory men and various administrators.

In those institutions which include both arts and agricultural colleges, it should be easy to arrange for such training. The pre-agricultural students should be taught in the same classes as the pre-medical students, or pre-engineering students, or straight arts students. In other places the students may not be given the formal arts classes but he should get the equivalent of them in a thorough broad training during the first two years. In all cases, if he is to benefit by his work, complete matriculation should be demanded before the student is permitted to enter. It is essential pedagogically that the basic subjects be taught before the student takes up the professional ones. They should certainly not be tacked on after the student has taken his professional work. This will involve an almost complete separation of the courses for technical agriculturists from those for the men who are to return to the farms. In short our ideal should be to bring our professional agricultural training abreast of our training for other professions by requiring as pre-agricultural study a large amount of, and as soon as possible complete, arts work. Only in this way can we secure a supply of properly trained research men as well as of other technical agriculturists.

The educational aspect of the relationship also involves the question of graduate work which is a very passing one in Canada. But as that is to be dealt with by another speaker, I shall refrain from discussing it.

I said at the outset that I would not attempt a comprehensive treatment of the subject—in spite of the time I have taken. I have tried to emphasize four respects in which, in my opinion, the relation of scientific research to agriculture is peculiarly important in Canada at the present time. They may be designated (1) Foundation, (2) Cooperation, (3) Remuneration and (4) Education.

W. P. THOMPSON
UNIVERSITY OF SASKATCHEWAN

LIMITATIONS OF EXPERIMENT IN EXPLAINING NATURAL HABIT, AS ILLUSTRATED BY THE DIURNAL MIGRATION[1]

THE general facts about the diurnal distribution of plankton organisms are these: at night there is greater abundance of a given species at higher levels and less abundance at

[1] A paper read at the meeting of the Western Society of Naturalists, Pasadena, California, June 20, 1919.

lower levels; by day the abundance is less at higher levels and greater at lower levels. One form may have a vertical range from the 100 fathom to the 200 fathom mark and another may range from the surface to 50 fathoms. The increase in numbers in the upper strata at night is believed to be due to upward movement of the animals from lower levels; the increase in numbers below the surface during the day is held to be due to descent from higher levels. The whole phenomenon is spoken of as diurnal migration.

The explanation of such movements has often been attempted. The one that has been most in vogue is based on change in direction of movement as some external condition is altered. That is, reactions are involved in the varying relations between the environment and the distribution of the organisms. Since the vertical movement is rhythmic and corresponds to the change between day and night it seems reasonable to suppose that the direction of movement changes as the light intensity changes; the animals might move toward a light of low intensity and away from one of higher intensity. Again, evidence has been presented showing that the geotropism of some forms changes with the light intensity. It has been thought, furthermore, that alterations in temperature or salinity are accompanied by reversals of direction of movement, either with relation to a source of light or to gravity. There is reason for this view since the organisms actually pass through different temperatures and salinities as they move up and down.

The foregoing possibilities all exist. But such questions as these have arisen: How generally do the possibilities apply? Are they applicable to all plankton animals? Do reversals in direction of movement really take place as it is possible they may? An investigation of such questions occupied the time of the writer for a year at the Scripps Institution. The reactions of different plankton animals were studied, with the special point in mind of applying the results to the diurnal migration problem. The attempt was made to find out for each species whether movement is toward or away from a weaker or stronger light; whether specimens ascend or descend in darkness as compared with diffuse light; whether locomotion in a vertical direction is toward or away from a light at the top of a column of water; whether direction of movement under any of the foregoing conditions changes if the temperature or salinity is changed.

It would be out of place to go into details here, but it should be said that experimental data that may show why the diurnal movement takes place were not obtained except in the case of the chætognath, *Sagitta bipunctata;* this form is one of seven that were used consistently. There is good evidence from field records that all of the species used in the experiments perform the diurnal migration. Furthermore, the field data from the San Diego region relating to one of the copepods and to *Sagitta* have been more completely worked out than for any other marine plankton animals anywhere. The relation between abundance or scarcity of these forms and external conditions revealed by collecting records, is thoroughly well known.

While the experiments that have been mentioned did not show how behavior in the laboratory is related to diurnal migration, they did bring to light matters that are certain to affect the interpretation of experimental results as applied to an explanation of a natural habit. It may be suggested, also, that these matters will probably enter into any study of reactions whose purpose is to discover the reasons for a given sort of behavior under natural conditions. The following are the points which seem to be of general importance.

1. Specificity in behavior. There is no doubt that a general explanation of diurnal migration is possible only when the different species have been studied in the laboratory; and it may not be possible even then to account for the diurnal migration as a general occurrence in the plankton. Various animals perform the migration together, but the reasons are different for the different forms. "Blanket explanations" have been given which are misleading because the reaction differences between species were not appreciated. One should not attempt to account for the vertical migration

of a copepod on the same basis that one explains a similar habit in a chætognath.

2. There is something connected either with the removal of specimens from the sea or their retention in the laboratory, or with both, that affects the responses in some cases. The relation between the behavior in the laboratory and that in the sea is consequently obscured. For example, the copepod, *Calanus finmarchicus,* can be taken at twenty fathoms during the day. When removed to the laboratory these animals are positive to light for about an hour on the average but become negative and remain so persistently. Is it not obvious that such a reversal makes it difficult to know how to apply experimental results toward explaining the migration? This kind of thing did not appear in all the species that I used, but it certainly should be looked for in all cases.

3. Specimens of the same species, but obtained from different locations or habitats, show noteworthy differences in behavior. A good example of this is found in the reactions of a copepod, *Acartia tonsa.* If animals are used that were obtained at the surface they move toward the light, and they ascend in diffuse light though they descend in darkness. But if the specimens were obtained from deeper water (60–100 feet) they move away from the light and descend both in diffuse light and in darkness.

Suppose that one is attempting to explain the diurnal migration of *Acartia.* The results of experiments with surface animals would lead one to expect to find the animals at the surface during the day and at lower levels at night. While they may be found at the surface by day at times, we have thousands of collections which show that these copepods are much more abundant at the surface at night. It depends on the particular animals collected whether it shall be said that *Acartia tonsa* as a species is positive or negative to light. It is worth noting in this connection that specimens of *A. tonsa* from deep water, if left in the laboratory for several hours, will react as surface animals do.

4. Need of experimental and field data together. It does not seem possible that the facts of nor the reasons for the diurnal migration (or any other natural habit) can be obtained without both laboratory and field studies. The results of work in the field will show what the animals do in their natural surroundings, while experimental work *may* show why they act as they do. Experiments are incapable of revealing what occurs in a natural habitat, unless it is possible to duplicate nature in the laboratory and at the same time secure experimental control. One can hardly do better in this connection than to quote what another has written:

> What right has one to assume that the reactions of an animal taken rudely from its natural habitat and as rudely imprisoned in some improvised cage are in any scientific sense an expression of its normal behavior either physical or psychical? Is it within the range of the calculus of probability that conclusions drawn from observations made upon an animal in the shallow confines of a fingerbowl, but whose habitat has been the open sea, are trustworthy? . . . Laboratory appliances are indispensable. But at the same time it must be recognized that they are at best but artificial makeshifts whose values, unless constantly checked by appeals to nature, must be taken at something of a discount.[1]

Such statements as the foregoing can not apply to experimental procedure that attempts to ascertain what animals can do, for example, or how sensitive they are to stimulation. Strictly physiological studies are both necessary and important and their brilliant results more than justify them. But in such methods of working the agent is emphasized rather than the organism, and the aim is to "work out the physics and chemistry of biological phenomena."[2] When, however, the organism is the chief interest, the natural history of the organism must be known if we are to study nature instead of things in a laboratory. Laboratory studies are of no more importance than those made in the field if one's object is to get

[1] Charles W. Hargitt, *Jour. An. Behav.,* 2, pp. 51–52, 1912.

[2] H. S. Jennings, *Amer. Jour. Psych.,* 21, p. 353, 1910.

knowledge of living things as they are in nature.

Calvin O. Esterly
Scripps Institution of the
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SCIENTIFIC EVENTS

THE BRITISH NATIONAL PHYSICAL LABORATORY

The British Department of Scientific and Industrial Research announces that the report of the National Physical Laboratory for the year 1919 has now been issued. It contains among other matter the report of the executive committee for the year 1919, the statement of work proposed for the year 1920–21, a list of papers published by the laboratory during the years 1917–18 and 1919 or communicated by members of the staff to scientific societies or to the technical journals, and the report of the director for the year 1919.

Some particulars of special work done during the war which it was previously necessary to treat as confidential are included, and the descriptions of such work now given will, it is hoped, be found of interest. This special work includes gauge testing carried out for the ministry of munitions, and a large number of special researches carried out by the electricity and metallurgy department and by the William Froude national tank.

The heads of the various departments give accounts of the recent work of the laboratory as follows:

Physics Department.
- I. Heat:
  - (*a*) High Temperature and General Work,
  - (*b*) Thermometer Testing,
  - (*c*) Oil Apparatus Testing.
- II. Optics.
- III. Radium and X Ray work.
- IV. Tide Prediction.
- V. Library.

Electricity Department.
Metrology Department.
Engineering Department.
Aerodynamics Department.
Metallurgy Department.
The William Froude National Tank.

THE FAIRPORT FISHERIES BIOLOGICAL STATION

The new building will be dedicated on October 7. The formal exercises will be divided into two parts—a forenoon session devoted to the immediate service of the station to industries and an afternoon session to consider the functions of the station in the advancement of science and the possibilities of further economic applications of its work. At each session there will be a principal address and three or four brief talks or messages. The speakers will be men of distinction in science and in public service. Opportunity will be afforded for inspection of the establishment, and, at suitable times to be appointed on October 7 and 8, scientists, state officers and other guests in attendance will be invited to confer regarding its purposes and work.

The reservation is on the Mississippi River, twenty miles below Davenport, and nine miles above Muscatine, Iowa. It comprises 60 acres of ground extending from the bank of the river to an elevation of about 200 feet above the river. Principal buildings are the biological laboratory, tank house, pumping station, hatchery (temporary laboratory), shell-testing plant and cottages. There are two water systems—unfiltered river water with storage reservoir of 2,000,000 gallons capacity and filtered water with gravity sand filter and low and high pressure cisterns. There are thirty-one ponds, of which nine are concrete-lined, while the remainder have walls and bottoms of natural earth.

The former laboratory building, opened in 1914, was of frame construction, about 100 by 50 feet, with two full stories, half basement and attic. It was destroyed by fire December 20, 1917, with the loss of a valuable library and many scientific records and specimens. The new building has the same location and approximately the same external dimensions as the old, but experience gained during occupancy of the first building and the resourcefulness and skill of the architect, have combined to make the new one superior in available space, convenience and serviceability.

The present structure, which is of concrete, stone, and brick, has a fully finished basement besides two full stories and a finished third story over the center and larger portion of the building. The present laboratory accommodations for 16 investigators can be extended by conversion of other rooms into laboratories. A well-lighted library, chemical laboratory, photographic room, museum, tank and aquarium rooms are other useful features of the building.

The architect of the building is James M. White, professor of architecture and supervising architect of the University of Illinois, who freely gave his professional services to the national government.

EXPEDITIONS OF THE BISHOP MUSEUM

THREE parties of the Bayard Dominick Expedition from the Bishop Museum are now in the field. The Marquesas Island party consists of Dr. Edward S. Handy, ethnologist; Dr. Ralph Lauton, archeologist; Dr. Forest B. H. Brown, botanist. E. W. Gifford and Wm. C. McKern are conducting an ethnographic survey of the Tonga Islands. R. T. Aitken and John F. G. Stokes, ethnologists, are undertaking an anthropological study of the Austral Island group. Two additional botanists are to be appointed in October.

The scope of the cooperative work of the American Museum of Natural History and the Bishop Museum, under the direction of Dr. L. R. Sullivan, has been expanded to include a comprehensive anthropologic survey of the people of the Hawaiian Islands. It will include studies of the Hawaiians, Japanese, Chinese, Portuguese, Koreans and Anglo-Saxons. Particular attention will be given to full blood and mixed blood Hawaiians and to skeletal remains in ancient burial caves.

It is announced that the Young Collection of Polynesian ethnological material, the result of twenty years' work in the society, Marquesas, Easter and Paumotu Islands by J. L. Young, has been obtained by the Bishop Museum.

SCIENTIFIC NOTES AND NEWS

SIR F. W. DYSON, astronomer royal, Greenwich, has been elected an honorary member of the American Astronomical Society.

PROFESSOR T. D. A. COCKERELL, of the University of Colorado, has been elected an honorary fellow of the American Museum of Natural History in recognition of his distinguished services to science.

DR. WILLIAM MANSFIELD CLARK, physical and biological chemist at the Dairy Division, Bureau of Animal Industry, U. S. Department of Agriculture, has become head of the chemical division of the Hygienic Laboratory.

DR. AMADEUS W. GRABAU, for eighteen years professor of paleontology at Columbia University, has been called by the Chinese government to a professorship at the University of Peking. He also has been appointed a member of the Chinese Geological Survey. Dr. Grabau will remain for three years in China to build up geological research work for the Chinese government.

MR. JAMES T. NEWTON, commissioner of patents, has resigned, after thirty years of service in the Patent Office.

MR. LESLIE SPIER, assistant in anthropology in the American Museum, has been appointed associate curator of the museum of the department of anthropology in the University of California.

DR. RODNEY B. HARVEY, formerly plant physiologist in the Division of Plant Physiological Investigations, Bureau of Plant Industry, who resigned to accept the position of assistant professor of plant physiology in the University of Minnesota and assistant plant physiologist in the Minnesota Experiment Station, has been retained on the rolls of the bureau as collaborator under a cooperative arrangement.

MR. HOYT S. GALE, who recently returned from Bolivia, has resigned from the U. S. Geological Survey, to take up private work.

BENJAMIN RICHARD JACOBS has resigned from the Bureau of Chemistry, U. S. Department of Agriculture, to become director of the

National Cereal Products Laboratories, with offices in Washington, D. C.

Professor Harold R. Hagan has resigned as professor of zoology and entomology at the Utah Agricultural College.

Dr. Charles E. Simon, professor of clinical pathology at the University of Maryland, has severed his connection with that institution and has accepted the position of a lecturer at the School of Hygiene and Public Health of the Johns Hopkins University. He has also been appointed managing editor of the forthcoming *American Journal of Hygiene,* of which Dr. William H. Welch is the editor in chief.

The Mary Kingsley medal has been conferred on Professor G. B. Grassi, professor of comparative anatomy at the University of Rome, for his research on the transmission of malaria by mosquitoes and the development of the hematozoa in the mosquito body.

Professor J. B. Farmer, professor of botany in the Imperial College of Science and Technology, London, has been appointed a member of the advisory council to the Committee of the Privy Council for Scientific and Industrial Research.

Mr. B. B. Woodward has retired from the British Museum (Natural History) but will complete his catalogue of the natural history library.

Professor Herbert E. Gregory, who by a cooperative agreement with Yale University is serving as director of the Bishop Museum. Honolulu, has returned to New Haven and will resume his university work for the first half of the present academic year.

Dr. S. I. Franz, of George Washington University and the Government Hospital for the Insane, represented the American Association for the Advancement of Science at the recent Cardiff meeting of the British Association. The General Committee has resolved that national associations for the advancement of science shall in future be invited to send representatives to meetings of the British Association.

W. S. Kew, on leave of absence from the U. S. Geological Survey, has left California for private work in Sonora, Mexico.

L. W. Stephenson has returned from Mexico and is acting chief of the Coastal Plains section of the U. S. Geological Survey, during the absence of T. Wayland Vaughan who attended the Pan-Pacific Scientific Conference in August and is spending the rest of the summer in study and correlation of the marine Tertiary strata of the Pacific coast.

Dr. F. W Traphagen, professor of metallurgy in the Dakota School of Mines, has returned to Rapid City, South Dakota, after spending the summer in metallurgical research work for the Denver Metals Co., at their plant at Utah Junction, Colo.

Dr. Lewis Wm. Fetzer has resigned as professor of physiology and pharmacology in the Baylor University College of Medicine, to take charge of the laboratories of St. Paul Sanitarium at Dallas, Texas.

A committee was organized in 1910 to collect funds for a monument to Lombroso. The committee had concluded its task when the war broke out but the execution of the monument was deferred. The *Journal* of the American Medical Association states that the matter has been taken up again and it has been found that the funds collected are inadequate for the purpose now. So the committee appeals for more donations. They can be sent to Professor Enrico Ferri at Rome. The sculptor is at work on the monument which will be unveiled at Verona in the spring of 1921.

Eric Doolittle, professor of astronomy in the University of Pennsylvania and director of the Flower Observatory died on September 21, from heart disease at the age of fifty years. Professor Doolittle succeeded his father the late Charles L. Doolittle in the directorship of the Flower Observatory in 1915.

Samuel Sheldon, for thirty-one years professor of physics and electrical engineering at the Brooklyn Polytechnic Institute, died on September 4 at the age of fifty-eight years.

MR. ARTHUR JACKSON ELLIS, geologist in the Water Resources Branch of the U. S. Geological Survey, died on July 22, 1920.

THE death of Charles N. Forbes, for twelve years curator of botany on the staff of the Bishop Museum, occurred on August 8.

DR. GEORGE MOREWOOD LEFFERTS, a retired specialist in throat diseases, emeritus professor of the College of Physicians and Surgeons, Columbia University, where he was a member of the faculty from 1874 to 1904, died on September 21 at the age of seventy-four years.

KARL HERMANN STRUVE, director of the Berlin-Babelsberg Observatory, and professor of astronomy in the Berlin University, died on August 12.

WE learn from *The Observatory* of the death of Mrs. Frametta Wilson, who was one of the five women pioneers admitted as fellows of the Royal Astronomical Society in 1916, and was later elected a member of the council. Mrs. Wilson had been awarded the "Edward C. Pickering Astronomical Fellowship for Women" for the college year 1920–21 had been assigned by the Harvard College Observatory.

DR. J. PIERRE MORAT, formerly professor of physiology at the Lyons medical faculty, has died at the age of seventy-five years.

THE British Thomson-Houston Company has decided to establish two scholarships, one of which will be allotted to Cambridge. It proposes to select from the engineering graduates of that university who have worked with the firm for not less than six months a scholar who will be sent to their American associates, the General Electric Company. The company proposes to allow for the student's expenses for one year an equivalent of $1,800 dollars. After a year's study in America he will be expected to return to the British company.

COLUMBIA UNIVERSITY, beginning with the autumn term, will offer in cooperation with Rutgers College and the State University of New Jersey a regular four years' course in agriculture leading to the degree of bachelor of science. The first two years will be given chiefly at Columbia and the second two years at Rutgers. The student who completes the course will receive his degree from Rutgers College. The requirements for admission are the same as those for Columbia College. Students are urged to spend at least a year on a well organized farm before entering Columbia. Working on farms during summer vacations approximates satisfactory farm experience.

UNIVERSITY AND EDUCATIONAL NEWS

THE first section of the new engineering shops which are being constructed at Camp Randall for the College of Engineering of the University of Wisconsin will be ready for occupancy about the first of the second semester. This building is the first step towards moving some of the engineering work to Camp Randall and it will relieve the overcrowded conditions resulting from the heavy enrollment in the College of Engineering since the close of the war.

DR. WILLIBALD WENIGER, formerly head of the department of physics, who left six years ago to engage in research work at the Nela Research laboratory of the National Electric Lamp Division of the General Electric Company, Cleveland, Ohio, has returned to his former position in the Oregon Agricultural College. At this institution Dr. Floyd E. Rowland, assistant professor of chemistry in the University of Kansas, has been appointed head of the department of chemical engineering, and Dr. Nathan Fasten, of the University of Washington, has been appointed associate professor of zoology. Dr. S. M. Zeller, assistant professor of plant pathology has been promoted to be associate professor in charge of orchard disease investigation.

DR. PHILIP HADLEY, formerly professor of bacteriology at the Rhode Island State College and biologist at the Agricultural Experiment Station, has received appointment on the faculty of the department of bacteriology and hygiene, school of medicine, University of Michigan.

Dr. Vernon K. Krieble, assistant professor of chemistry at McGill University, succeeds Dr. R. C. Riggs as Scoville professor of chemistry at Trinity College, Hartford, Conn.

DISCUSSION AND CORRESPONDENCE

ELECTRICITY AND GRAVITATION

The action of gravitation on light is generally regarded as a continuous process but if we consider a ray of light as the limit of a chain of rectilinear rays for each of which the velocity has its upper limit value c, we can regard the gravitational action on the ray as built up of a succession of impulses, each of which changes the direction of the ray. To obtain a definite picture of this action, let us imagine the æther to be built up of electrical doublets travelling along straight lines with velocity c and sometimes colliding with one another. A collision in which the doublets break up and their constituents secure new partners leads to a temporary manifestation of free electric charge. For simplicity we shall suppose that this type of collision takes place only at points where matter is present and that such collisions occur continually so that the manifestation of free electric charge is permanent[1] and approximately steady. At a point not occupied by matter a collision may be supposed to result simply in a change in the direction of motion of the doublets. It is possible, however, that collisions are all of the first type. The elementary type of electromagnetic field is one in which a doublet breaks up into positive and negative constituents which fly away in different directions with the velocity c. The field of an electric charge moving with a velocity less than c can apparently be built up from such elementary fields by superposition and so the assumption of the fundamental character of the elementary field seems legitimate.

From the elementary fields it is possible to build up a type of field in which the electric charge associated with an electric pole fluctuates owing to the fact that the constituents of a doublet are in the neighbourhood of the pole at slightly different times. We shall assume that the electric action between two poles depends on the instantaneous values of the charges and shall endeavor to estimate the effect of the fluctuations. Let us assume that the total number of doublets which break up at an electric pole per unit time is proportional to the mass associated with the pole. This number will also be supposed to be the number of doublets which are created from the constituents of those which break up. Among the doublets which arrive at the second pole B there may be some that have come from A. Let us suppose in the first place that there is no gravitational shielding, then it seems reasonable to assume that the percentage of B's doublets which have come directly from A is proportional to the number which leave A and so is per unit time, proportional to the mass of A. The number of doublets which pass directly from A to B per unit time is thus proportional to the product of the masses of A and B. The doublets themselves will be supposed to be so small that the emission of the different doublets and the arrival of others may all be regarded as independent events. At an instant of time t when a doublet from A is arriving at B the charge on B may be then regarded as equal to $e' + f(t)$ when the charge on A at the earlier time $t - (AB/c)$ was $e - f(t)$. The function $f(t)$ is supposed to have a mean value equal to zero so that e and e' may be regarded as the mean charges associated with A and B respectively. The above expressions for the charges are supposed to hold only for the very short periods of time when the particular doublet under consideration is in the neighborhoods of B and A, at other times the values of the charges are governed by the presence of other doublets.

The mean value of the electric force between A and B over a small period of time, which is

[1] We imagine one component of a doublet to be momentarily separated from its fellow, when another doubtlet comes along the lonely charge secures a new mate and leaves another charge all alone, this charge behaves in a similar manner when it encounters another doublet and so on. In what follows we really consider collisions between doublets and free electric charges.

very large compared with the time during which a particular doublet is in the neighborhood of B, is proportional to $ee' - k^2$ where k^2 is the mean value of the square of $f(t)$ for all the doublets which pass from A to B and arrive at B in this interval. In accordance with our previous hypothesis it seems reasonable to conclude that k^2 is proportional to the product of the masses of A and B.

If in the interval of time from t to $t + dt$, no doublets arrive at B while a doublet left A in the corresponding interval $t - (AB/c)$ to $t + dt - (AB/c)$ it is clear that the mean value of the electrical force between A and B in this interval depends on ee' and there is no gravitational action. Other cases may be considered in a similar way and it is clear that the gravitational action depends only on the doublets which go directly from A to B. The action of B on A depends likewise on the doublets which go directly from B to A.

The present theory indicates that there may be a slight screening effect when a third body C is interposed between two bodies A and B, for C may be supposed to receive some of the doublets which would ordinarily go directly from A to B or vice-versa. The recent work of Nipher[2] and Majorana[3] thus becomes of additional theoretical interest when it is considered in the light of the present theory.

Gravitational action may be slightly modified, too, by collisions between doublets travelling with velocity c. In this connection it may be worth while to point out that if P and Q are two doublets travelling along different straight lines with velocity c, then after a certain instant it is possible for a particle travelling with velocity c to meet first one doublet, say P, and then Q but not for such a particle to meet first Q and then P. A series of moving doublets may thus be arranged in a definite order; something which happens to one doublet may affect those which come later in the series but not those which come earlier. This result may have some connection with the damping of oscillations in the emission of light. A more imperfect form of the present electrical theory of gravitation has already been published in *Proc. London Math. Soc.*, T. 18 (1919), p. 95, and in the *Messenger of Mathematics*, T. 48, p. 55. The possibility of a connection with the work of Einstein and Majorana has not been pointed out previously. The present theory seems to be free from the objections raised against the older electrical thory of gravitation (see O. W. Richardson, "The Electron Theory of Matter," p. 596), there may, however, be some other fatal objections to it.

H. BATEMAN

[2] SCIENCE, September 21 (1917).

[3] *Phil. Mag.*, T. 39, May (1920), p. 488.

PROTOZOA IN SAWDUST FOR CLASS WORK

IN studying the method of excreta disposal by compositing night-soil with sawdust, the chance observation was made that microscopic examination of old sawdust piles revealed the presence of *Euglypha* cysts. Samples of sawdust were used for experimental culture of hookworm eggs and it was observed that the cultures showed profuse contamination with amœba, flagellates, ciliates, and free living nematodes. Samples from old sawdust piles were then moistened and incubated with the result that numerous specimens of protozoa and free nematodes were found.

The sawdust used was chiefly from southern pine.

This note is published with the thought that it may be of practical service to teachers in providing material for class work.

C. W. STILES

U. S. PUBLIC HEALTH SERVICE

CONCERNING DIASTROPHISM

Two papers have appeared during the current year which once again bring before American geologists the vexed question of systemic boundaries. In the first Böse[1] concludes that the ammonites found at Tularosa, New Mexico, 200 feet above the base of the Abo sandstone, are of Carboniferous age. This inter-

[1] Böse, E., *Am. Jour. Sci.*, Vol. 49, pp. 51–60, January, 1920.

pretation involves considering the break in sedimentation at the bottom of the Manzano group of that region, of which the Abo is the lowest formation, as having occurred during so-called Pennsylvanian time, and would place the boundary between the Pennsylvanian and the Permian at some undetermined horizon within the Manzano group of conformable formations, as Lee[2] has pointed out. I would not be in the least surprised if this last is not the true interpretation of the facts. It is just what would be expected by any one disposed to doubt the validity of the so-called diastrophic method of correlation, and, if I mistake not, is exactly what occurs in the Appalachian region.

The second paper to which I refer is one by Twenhofel[3] who concludes that all of the Cretaceous below the Benton in Kansas should be referred to the Comanchean, and that the Cheyenne-Kiowa-Medicine sequence of southern and the Mentor-Dakota sequence of central Kansas are the equivalents of the Washita division of the Texas Cretaceous, although it is conceded that the Washita faunas and floras are probably of Cenomanian age and therefore Upper Cretaceous according to European chronology.

Without discussing the merits of these conclusions in this place, I wish to call attention to the more general question involved, which is clearly recognized by Lee in connection with the Manzano group, and which is discussed at some length by Twenhofel in connection with the Kansas Cretaceous, namely as to what are the criteria of systematic boundaries.

I can see no fundamental objection to using such terms as Comanchean as a convenient group or descriptive term any more than in using such terms as Mississippian or Pennsylvanian, disregarding even that Mississippian in its original significance was Cambrian, but to use Comanchean as the equivalent of the European Lower Cretaceous, which it is not in either its lower or upper limits, and thus to bring about a situation where Lower Cretaceous in Europe, Asia, Africa, Australia, and South America means Lower Cretaceous, whereas Lower Cretaceous in the United States means early Upper Cretaceous, appears to me most reprehensible.

American geological literature has been deluged, one might almost say diluted, with diastrophism during the last decade, and philosophers, scientific as well as political, stand on every street corner. Whether there is any world wide periodicity in movements of the strand line as Suess and Chamberlin contend, or whether each region has its individual history as Willis contends, I do not know, although what we know of geological history is all in favor of the latter supposition. I should imagine that sometimes one and sometimes the other might be true, depending entirely on the causes that affect the relative positions of the land and sea in specific cases.

I certainly can see no basis for the "law of periodicity" that Willis writes about beyond the partial fulfillment of Newberry's much older conception of cycles of sedimentation, which are no more comparable in chronologic magnitude than are the life cycles of organisms.

If American geology is to finally adopt diastrophism as the ultimate basis for the delimitation of the more important time boundaries, and it is already clear that the geologists of no other nation are likely to follow our lead, we shall have to devise a different terminology for each continent, or even for different parts of the same continent. For example on our Pacific coast there were Triassic floodings that have been successfully correlated by Smith on the basis of their ammonite faunas with those of the Mediterranean region of the old world. On our Atlantic coast there are no traces of any corresponding events. Exactly similar conditions prevailed in the two general regions during the Jurassic. Lower Cretaceous marine sediments are confined to the southern and western parts of North America, and one might start at the bottom of the geological column and point out very many similar contrasts.

The two continents whose geology has been

[2] Lee, W. T., *Idem*, pp. 323–326, May, 1920.

[3] Twenhofel, W. H., *Idem*, pp. 281–297, April, 1920.

longest studied and is best known, namely Europe and North America, show the greatest amount of divergence even in formations as recent and well preserved as those of the Tertiary, and there are the most striking contrasts in the geological history of Mediterranean as compared with central and northern Europe. The same situation exists upon all of the other continents.

Even supposing that changes in relative level have been due to general causes such as the periodic sinking of ocean basins and the filling of oceans by sediments,[4] which has not been demonstrated and is directly opposed by what we know of geological history, the results as reflected in those chapters of geological history available for our study, would vary with the initial attitude of the land in a particular area, its location with respect to the position of the antecedent sea level, etc. A striking illustration of this is furnished by a comparison of the not very remote regions of Belgium and the Paris basin, or even of the center and periphery of the latter during the Cretaceous and Tertiary.

The question resolves itself into whether geological classification in its major outlines shall be local, that is, provincial and nationalistic, or whether it shall be understood and capable of application in any country. I am one of those reactionaries who believe that classification is a means and not an end, and that, however imperfect the scheme may be as devised for the region first and longest studied, namely Europe, the classic names with the historic perspective that goes with them, should be adhered to in this country.

Classifications are all purely artificial, they are the medium of exchange, and geological time boundaries are no more physical facts than are political boundaries, even though it may be difficult to avoid thinking of them as though they are entities. Probably the best ultimate solution would be to have a universal (international) time scale and a local sedimentary scale as we now have for our formational units. After all the best classifications, whether of geological time, systems of rocks, organisms or igneous rocks, are those most easily understood and used, those in which facts and relationships are not obscured or wholly disguised by names.

Time is continuous, boundaries are always subjective, and the Permian, Triassic, Lower Carboniferous or Lower Cretaceous are to me as essential to clear thinking and the interchange of ideas among nations as are the minutes, hours, days and weeks of current chronology, however illogical these might seem in sidereal astronomy.

The problem of correlation would be immensely simplified if diastrophism could be demonstrated to be of universal application. This is I believe the reason it has so appealed to many, but like its prototype devised by Werner it is altogether too simple to be true.

It seems to me that the most reliable basis of correlation must remain paleontologic until such time as it can be shown that changes of relative elevations are due to changes of sea level, and if this be true there is a disturbing factor in attempting to settle the relative merits of homotaxis versus synchroneity. I have a feeling, however, that homotaxis, although theoretically true, has been greatly overestimated in its bearing upon our interpretations of geological history where we do not have continuous sedimentation to deal with.

Paleontologic correlation, it should be needless to remark, rests ultimately upon the synthesis of all classes of organic evidence, not merely upon invertebrates, vertebrates or plants. How little this truism is observed in practise and to what an extent geological thought is still permeated by Cuvier's cataclysmal philosophy can be appreciated by reading any recent discussions of the boundary between the Devonian and Carboniferous, the Triassic and Jurassic, the Jurassic and Lower Cretaceous, or the Upper Cretaceous and Tertiary. Accepting the doctrine of evolution for life and of uniformitarianism for earth history, the average stratigraphic paleontologist seems determined to prove cataclysms and special creation.

[4] A simple quantitative computation will show how trifling would be the maximum change in sea level from the latter.

The ultimate solution, or at least the one that is most to be desired, it seems to me, will be a universal time scale which shall have its basis in paleontology and shall adhere to the classic names, and in which the cycles and epicycles of diastrophism will be regarded as probably the most useful criteria for delimiting formational or larger sedimentary units, but never *per se* as criteria for the division of geological time.

Edward W. Berry
The Johns Hopkins University

SPECIAL ARTICLES

THE INFLUENCE OF DRY VERSUS FRESH GREEN PLANT TISSUE ON CALCIUM ASSIMILATION

In early work on mineral metabolism with both the cow and the goat we showed that milking animals, receiving grains and dry oat straw as a roughage, are brought into a decided negative calcium balance. In the case of a goat the interesting observation was made that after a period of negative calcium balance, followed by access to fresh green grass, a positive calcium balance was observed, using the same ration as was used in the period preceding the access to green plant tissue. In extensive experiments Forbes and associates and Meigs and his associates have observed negative calcium balances with milking cows receiving rations liberally supplied with calcium. The rations used were from air *dried* materials, supplemented in some cases with silage. The striking feature of all the data obtained in these experiments was the large amount of fecal calcium, indicating a failure to assimilate satisfactorily this base.

In these our preliminary experiments, we have used milking goats. They have readily been brought into negative calcium balance on a ration consisting of air dried grains and air dried straw, with more calcium excreted in the fecal residue than was ingested with the ration. When the *dry* cereal straw was displaced by an equivalent in dry matter of fresh *green* material, with no increase in the total calcium intake, the negative calcium balance was reduced in one animal from 1.6–2.7 grams CaO to .6 CaO per day. With another animal it was reduced from 1.5–2.5 grams CaO per day to .3–.8 gram per day. On the low calcium intake of 8 to 9 grams of CaO per week we could not expect a positive calcium balance to ensue, but this remarkable difference in the amount of calcium assimilated from the two rations we believe, has very great significance.

These changes in calcium assimilation are not to be attributed to variation in water intake or to unavailability of the calcium. Apparently there is something having its source in fresh green materials, which controls or assists calcium assimilation. It is suggested that under the extra strain of rapid growth or milk production not enough calcium can be assimilated for the liberal uses made of this element, unless there is present an abundance of calcium in the diet as well as an abundance of this something that assists calcium assimilation. Possibly we are dealing with the anti-rachitic vitamine, assumed as the fourth food accessory factor. In any case this problem touches growth, milk production and egg production. In the case of nursing women the relation of diet to a positive or negative calcium balance and to dental conditions will assume new aspects.

The supposition that we are dealing with something influencing calcium assimilation and which is more abundant in green than in dried plant tissue and consequently variable with the season's milk, would explain the variations in the seasonal frequency of rickets, as observed and commented upon by Hess.

Our data are not yet inclusive enough to indicate definitely the factors involved in this problem, yet we have been sufficiently impressed with the constancy of the observations made that it appears desirable to reemphasize this relation to mineral metabolism which we anticipated some years ago and expressed in an earlier publication.

E. B. Hart,
H. Steenbock,
C. A. Hoppert

THE AMERICAN CHEMICAL SOCIETY. IX

DYE SECTION

Charles L. Reese, *chairman*

R. Norris Shreve, *secretary*

Dye patents of the chemical foundation, incorporated: S. A. TUCKER. The paper deals with the classification of the dye patents owned by the company which forms the largest division of any class of its patents, and covers practically all classes of dyes. The method followed enables one to locate any patent either by the chemical constitution of the dye or its application. Patents actually licensed either by the Chemical Foundation or the Federal Trade commission are discussed at considerable length. A list of patents of which the Schultz number is known but which have not yet been licensed is given. The advantages in the form of license as issued by the Chemical Foundation are pointed out and the influence which the company may have on the American chemical industry is discussed.

Dyes for photographic sensitizing: W. F. MEGGERS and F. T. STIMSON. Illustrations are given of the photographic impression made by a continuous spectrum upon Seed's 23 plates before and after treating with typical photo-sensitizing dyes. Spectral transmission curves of the dye solutions and spectral sensitivity curves of photographic plates stained with the dyes are shown. Examples of the use of color sensitive photographic plates in aerial photography are exhibited and the importance of photo-sensitizing dyes to future developments in photography is emphasized.

Anthranilic acid, tests and purity of the commercial product: J. F. RAGSDALE. The melting point of anthranilic acid is found to be higher than that usually given in the literature. Since decomposition occurs on melting, certain precautions must be observed in taking the melting point. Various methods for determining the anthranilic content of the commercial product are discussed, and diazotation with standard nitrite solution is recommended. Several methods for standardizing the nitrite solution have been tried, and advantages and disadvantages of each are pointed out.

Ortho chlor para toluene sodium sulfonate—purity and tests of the commercial product: L. A. WATT. Ortho chlor para toluene sulfonate is one of the dyestuff intermediates now produced in quantity in this country. The impurities encountered in the commercial product are enumerated and a procedure for their detection and determination based on the relative solubilities in alcohol is suggested. Data obtained from the examination of both low grade and typical commercial products are given.

Uses of formic and oxalic acids in the dyeing industry: LANCELOT W. ANDREWS. For the acidification of dye baths, an acid should be selected having intrinsic strength enough to cause full utilization of the dye and fixation on the textile, but not having so great intrinsic strength as to injure the fabric. The intrinsic strength of the acids are determined by their electro-dissociation coefficients. It is shown by both theory and practise that formic acid is particularly well suited for use of the dyer and that its high dissociation constant and its low molecular weight, render its employment thoroughly economical. Formic acid offers the following advantages as compared with the mineral acids, or with acetic and other available organic acids: (1) No tendering of the textile, (2) greater evenness of dyeing, (3) better exhaustion of the dye, (4) better fixation on the textile, (5) brighter color, (6) better penetration, (7) economy. American manufacturers are now able to supply both formic and oxalic acids, and the mordant salts of these acids, with Al, Cr, Sn, Sb, etc.

Synthetic dyes as applied to chemico-therapy and microscopy: GEO. HEYL. Part 1. Medicinal Dyestuffs: (*A*) Theory of action. (*B*) Synthesis and pharmacological effect. (C) Classification of medical dyestuffs: (*a*) Eutherapeutic and dytherapeutics; (*b*) Dyestuff specifics (salvarsan, acriflavine, etc.); (*c*) External dyestuff antiseptic; (*d*) Dytherapeutics and parasiticidal action; Trypan blue, trypan red, naga red, methylene blue, etc.; (*e*) Neutral dyes and nerve cells; Methylene azur, eosine-azurs, etc., and neutral red. (D) General outline of preparation. Part 2. Microscopic Stains: (*A*) Theory of action (Ehrlich-Witt). (*B*) Classification. (*C*) Standardization.

Dye research: ROBERT E. ROSE. Research we must have—not a mere checking up of receipts, not a mere search for information which is known to others. We must graduate to real research, that is, enter the entirely unknown. Much of this type of research must be done in the universities, but under conditions of close cooperation with the

industries. To do this latter, the industries must lose their intense secrecy, and open their doors—with proper safeguards—to the university men. Raw products, money, commercial organization, industrial chemists, all these we may have and yet fail if we have not real research.

The hydrogen exponent classification of indicators and some of its applications: I. M. Bernstein. (1) Definitions of terms used. (2) Determination of indicator constants: (*A*) Approximation method of Salessky; (*B*) colorimetric method; (*C*) Bjerrum's method. (3) Classification of indicators according to exponents: (*A*) Chart of same showing color changes; (*B*) comparison with Glaser's classification. (4) Application of classification of volumetric analysis: (*A*) Neutralization curves. (*B*) Selection and concentration of correct indicator. (*C*) Preparation of new indicators. (5) Application of classification to biological technic: (*A*) Determination of hydrogen ion concentration; (*B*) the selection of correct indicator for physiological solutions.

The analysis of aromatic nitro compounds by means of titanous chloride: F. L. English. The procedure for the analysis of aromatic nitro-compounds as recommended by Knecht and Hibbert (see "New Reduction Methods in Volumetric Analysis") has been modified and successfully applied to numerous typical nitro-compounds of the benzene series among which are *m* and *p*-nitranilines, *o*- and *p*-nitrophenols, *o*- and *m*-nitro-*p*-toluidines, two of the nitro-salicylic acid isomers, dinitrobenzene, toluene and xylene, trinitrotoluene and picric acid. The experimental figures given show the method to be accurate to about 0.1 per cent., total error. Results are given also to prove the inapplicability of the method in its present form to nitrochlor derivatives such as *o*- and *p*-nitrochlorbenzene and dinitrochlorbenzene, under-reduction resulting in the first two cases and over-reduction, probably at the expense of the chlorine atom, in the last. When the chlorine is in the side-chain, however, the reaction proceeds more nearly quantitatively as illustrated by *p*-nitrobenzyl chloride and dinitroxylyl dichloride (1–3–CH_2Cl–2–4–NO_2 benzene). The analytical procedure, as well as the preparation, standarization and storage of the volumetric solutions, is described in detail.

Some physical constants of pure aniline: C. L. Knowles. A sample of aniline was carefully purified by conversion to the oxalate, regeneration and repeated vacuum distillation. Physical constants were determined immediately without undue exposure to the air. Freezing and boiling point determinations were recorded by means of a recently standardized platinum resistance thermometer. The constants found are as follows:

Freezing point — 6.24° C.
Boiling point 184.32–184.39° C. at 760 mm.
Specific gravity 15/15 1.0268.
Refractive index 20° C. 1.5850.

On exposure to the air aniline absorbs moisture very rapidly, taking up 2.4 per cent H_2O in 46 hours reducing the freezing point nearly 2° C. Indications are that the freezing point is the best criterion of purity which may be calculated by substitution in the following formula, (t) being the observed freezing point and (x) the per cent. aniline in the sample. $X = 108.79 + 1.41\ t$.

The absorption spectra of the nitric esters of glycerol: E. Q. Adams.

Tetramethylquinolines: L. A. Mikeska.

Naphthalene sulfonic acids. Some difficult soluble salts of naphthalene sulfonic acids: J. A. Ambler.

A method for the qualitative detection of some naphthalene sulfonic acids: J. A. Ambler.

Synthesis of s-Xylidine: H. L. Haller.

Alkali fusions. II. The fusion of sodium benzene disulfonate with sodium hydroxide for the production of resorcinol: Max Phillips and H. D. Gibbs.

A synthesis of thymol from p. cymene: Max Phillips and H. D. Gibbs.

The vapor pressure of phthalic anhydride: K. P. Monroe.

A new source of furfural and an investigation of the preparation and properties of "furfural green": K. P. Monroe.

The absorption spectra of the nitric esters of glycerol: Elliot Quincy Adams. Hepworth (*Jour. Chem. Soc.*, 115, 840–47 (1919)), from a study of the absorption spectra of the nitric esters of glycerol, concludes "(6) There does not appear to be any numerical proportionality between the number of hydrogen atoms of the hydroxyl groups displaced by nitro-groups and the degree of absorption for any particular dilution." A critical study of the results of Will (Ber. 41, 1107–23 (1908)), which have been accepted by Hepworth, indicates that his designations α- and β- for the mononitrates should be interchanged. Each nitrate radicle has an absorptive effect de-

pendent on its location in the molecule, but independent of the presence of other nitrate radicles.

Tetra methyl quinolines: L. A. MIKESKA and E. Q. ADAMS. The dicyanines—a series of photosensensitizing dyes extending the sensitiveness of the photographic plate farther into the infra-red than any other known substances—requires as intermediates the quaternary halides of 2, 4 dimethylquinoline and 2, 4, 6-trimethylquinoline. These bases are made by condensing with paraldehyde and acetone—by a synthesis similar to that of Shraup—respectively aniline and *p*-toluidine. The xylidines give by this condensation tetramethylquinolines, of which only one has heretofore been prepared (and incorrectly named). Three of the six possible isomeric tetramethyquinolines have been prepared: 2, 4, 5, 7-; m.p. 59°; 2, 4, 6, 8-; m.p. 86° and 2, 4, 5, 8-. The preparation of the others is in progress.

Naphthalene sulphonic acids. I. Some difficultly soluble salts of certain naphthalene sulphonic acids: JOSEPH A. AMBLER. The preparation and properties of the alpha naphthylamine salts, and of the beta naphthylamine salts of the naphthalene alpha, beta, 1–5, 1–6, 2–6, and 2–7 sulphonic acids, and of ferrous naphthalene beta sulphonate are given. Crystallographic-optical properties as determined by E. T. Wherry are included. These salts are all difficultly soluble in water and possess characteristic optical properties.

Naphthalene sulphonic acids. II. A method for the qualitative determination of some of the naphthalene sulphonic acids: JOSEPH A. AMBLER and EDGAR T. WHERRY. It was found that the properties of the salts described in the preceding article can be used for a qualitative test for the naphthalene alpha, beta, 1–5, 1–6, 2–6, and 2–7 sulphonic acids. Naphthalene beta sulphonic acid is detected with ferrous chloride; the 1–5 sulphonic acid, with alpha naphthylamine hydrochloride in boiling water; the 2–6 sulphonic acid, with beta naphthylamine hydrochloride in boiling water; the alpha acid by the solubility of its beta naphthylamine salt in hot acetone; the 1–6 and 2–7 sulphonic acids, by the optical properties of their beta naphthylamine salts.

Benzene disulfonic acid from benzene monosulfonic acid: C. E. SENSEMAN. The well-dried barium salt of benzene monosulfonic acid is treated with sulfuric acid varying in concentration from 93–98 per cent. The temperatures for the various runs are 220°, 250° and 280°. The quantities of acid used range from 1½–8 mols to one mol of the free monosulfonic acid. The duration of the various experiments was from 8–10 hours. Vanadium pentoxide and sodium sulphate were tried out as catalysts. The progress of the reaction was determined in each case by removing a sample at the end of each hour and analyzing for the disulfonic acid. Without the use of a catalyst the highest yield obtained was 93.3 per cent. A yield of 98.2 per cent. was obtained when sodium sulphate was used as a catalyst.

Synthesis of s-xylidine: HERBERT L. HALLER and ELLIOT Q. ADAMS. S-xylidine was synthesized (1) from mesitylene—which was oxidized to mesitylenic acid; changed to the amide; and finally converted by means of Hofmann's reaction to *s*-xylidine; (2) from *m*-4 xylidine by successive acetylation, nitration, saponification and deamination; and reduction. In the latter procedure the saponification and deamination were carried out in one step. Acetyl derivatives of the xylidine obtained by the two methods were compared, and their melting points found to be the same, 139.6°–140.2° (corr.). The optical properties also were found by Dr. E. T. Wherry to be identical.

Alkali fusions. II. The fusion of sodium benzene m-disulphonate with sodium hydroxide for the production of resorcinol: MAX PHILLIPS and H. D. GIBBS. Although the process of making resorcinol by the fusion of sodium benzene *m*-disulphonate with sodium hydroxide has been in use for a considerable number of years, nevertheless, there appears to be no agreement as to what are the proper conditions for conducting the fusion in order to obtain the highest yield of resorcinol. Using an apparatus especially suited for this work, a study was undertaken for the purpose of ascertaining the optimum conditions for conducting the alkali fusion. The problem resolved itself into a study of the four following factors: (1) Ratio of sodium hydroxide to the sodium benzene *m*-disulphonate; (2) the proper fusion period; (3) the proper fusion temperature; (4) effect of the addition of water to the sodium hydroxide upon the yield of resorcinol. The results obtained indicate that 310° is the best fusion temperature, two hours the best fusion period, 14 to 16 moles sodium hydroxide to one mole sodium benzene *m*-disulphonate the best fusion mixture, and that water when present in the fusion mixture tends to reduce the yield of resorcinol.

A synthesis of thymol from p-cymene: MAX PHILLIPS and H. D. GIBBS. p-cymene, an aromatic hydrocarbon, although long known to chemists,

has recently attracted considerable attention because of the fact that it is obtained in rather large quantity as a by-product in the spruce pulp industry. Thymol, which is of considerable commercial importance and which bears the same relation to *p*-cymene as does phenol to benzene, can not be obtained from *p*-cymene by the ordinary methods of getting a phenol from its corresponding hydrocarbon. However, by using the following rather indirect method the synthesis of thymol was accomplished: *p*-cymene was nitrated and the compound with the nitro group in the position ortho to the methyl group was obtained. This compound was reduced to the corresponding amino *p*-cymene or cymidine, which when sulphonated gave cymidine sulphonic acid, the sulphonic group entering the position ortho to the isopropyl group. The amino group of cymidine sulphonic acid was then removed through diazotization and subsequent reduction with alcohol and copper powder. The cymene sulphonic acid thus obtained was converted into its sodium salt and the latter when fused with sodium hydroxide was converted into thymol.

The vapor pressure of phthalic anhydride: K. P. Monroe.

The preparation of furfural: K. P. Monroe. Corn-cob adhesive prepared according to the method of La Forge and Hudson (*J. Ind. Eng. Chem.*, 10 (1918, 925) by extracting corn-cobs with water under pressure at 150° C. is an excellent source for furfural, since the gums so obtained are rich in pentosan and yield 26 per cent. of pure furfural. The essential improvements over the previously published methods of obtaining furfural from pentosan containing material are (1) removal of furfural from the reaction mixture by a rapid current of steam during the hydrolysis by 25 per cent. H_2SO_4; (2) separation of furfural from the dilute aqueous solution which constitutes the distillate by distillation with the acid of a fractionating column. Uses for furfural in the industries and arts are outlined; among these are the preparation of: hard resins similar to the well-known "Bakelite," soluble resins which may find application in the varnish trade, a series of dyes which may be obtained by condensation with various coal-tar products.

The vapor pressure of phthalic anhydride: K. P. Monroe. The vapor pressure of phthalic anhydride was determined by the static isoteniscope method of Smith and Menzies (*J. Am. Chem. Soc.*, 32 (1910), 1412–59). The following equation, of the type obtained by integration of the Clausius-Clapeyron equation with simplifying assumptions, was found to express the results:

$$(1) \qquad \log_{10} p = 7.94234 - \frac{2823.5}{T}$$

where p = vapor pressure in millimeters of mercury and T = absolute temperature. The molar latent heat of vaporization was calculated to be 12,910 colories, while the value 13.6 was obtained for the entropy of vaporization divided by R (the gas constant) at the temperature (near 218°) at which the concentration of vapor is 0.00507 mole per liter. According to the criterion of Hildebrand (*J. Am. Chem. Soc.*, 37 (1915), 970), this indicates phthalic anhydride to be a *normal* liquid.

The present independence of American synthetic dyes and how it was accomplished: Robert E. Hussey. Prior to the war all of the biological dyestuffs used in the bacteriological laboratories and in the laboratories of public health came from Germany. The two chief difficulties upon the cessation of imports that confronted the American manufacturer were lack of raw supplies and lack of accurate information as to their manufacture. All of these dyes must be absolutely uniform and standardized as certain arbitrary amounts are used in certain dilution to attain specific results. The army had to be supplied. Quantity production took place and, after much experimentation, this experience has now made it possible to supply scientific dyes equal and in some cases, superior to those formerly imported. Investigators should mention that American dyes were used to obtain their result for by this method proper encouragement to this industry would be given.

Charles L. Parsons,
Secretary

(*To be continued*)

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